Geissler thermometers, showed for a spherical bulb an increase of 0.16, and for a cylindrical bulb an increase of 0.27, of a degree Fahrenheit, for an additional atmosphere of pressure. Clearly, the amount of increase will depend upon the nature of the glass bulb, its thickness, size, and shape.

Many observations on vapor-pressure, on boiling-points under increased or diminished pressure, meteorological observations at unusually high stations or in mines, are subject to this correction; and, as no general correction will be satisfactory, each thermometer will have to be separately tested.

We have written to the signal-service bureau for information on this subject, and find that they 'have the matter under consideration,' and are making experiments. Besides, we have been referred to papers by Loewy in Proceedings of the Royal society, 1869, and by Marck, International bureau of weights and measures.

We write now to point out this source of error to readers of *Science* who may not have noticed it, and to ask if any can refer us to further memoirs and observations on the subject.

F. P. VENABLE.
J. W. GORE.

University of North Carolina, Jan. 23.

Is the dodo an extinct bird?

Since the publication of an article of mine upon the origin of birds, which appeared in the *Century magazine* for January, 1886, there have come to me a number of interesting letters questioning the fact that the dodo is entirely extinct. From among them I select one recently received from Dr. William Barr of Bovina, Miss. My correspondent tells me that he clipped not long ago, from an English newspaper, the following item: "Mr. Manley Hopkins, consul-general of Hawaii, writes to an English journal, 'By my papers received from Hawaii, I observe that among some birds brought by the schooner Fanny from the Samoan group was a single specimen of that *rara avis in terra*, the dodo. I am sure your readers will be interested to hear that this bird, supposed to have become extinct more than a century ago, still lingers in the little-explored Samoan Islands of the South Pacific.'"

A number of continental naturalists, who, no doubt, have arrived at their opinions through the rumors brought home by explorers, have predicted that the dodo will some day be found to be one of the forms of the existing avifauna of the island of Madagascar.

R. W. SHUFELDT.

Fort Wingate, N. Mex., Jan. 20.

Evidences of glacial action on the shores of Lake Superior.

Evidences of glacial action are abundant about Peninsula Harbor, on the north shore of Lake Superior. The tops of the low islands, and of the hills along the shore, are rounded in a striking manner. Below the surface of the water well-preserved grooves and scratches extend in a general north-east and south-west direction. The crevices in the granite rock which extend across the glacial markings have their northerly sides nearly intact, while the sides opposite are considerably worn. Where the crevice extends in about the same direction as the glacial mark, both of its sides are gouged out.

On Verte Island, Nipígon Bay, Lake Superior, a well-preserved beach of water-worn pebbles lies, as near as could be determined by rough measurement, two hundred and eighty feet above the present level of the bay.

A. A. CROZIER.

Grand Rapids, Mich., Jan. 26.

Professor Newcomb's address before the American society for psychical research.

In view of the utterances in the last two numbers of *Science*, called forth by my address before the American society for psychical research, some comment by me may not be inappropriate.

Of the two criticisms upon my address, which are put forth in the comments of Jan. 22, one seems to me well founded. It is that directed against my definition of thought-transferrence as something which is supposed to take place without any physical connection between the acting and the percipient minds. *Science* correctly points out that the absence of a physical medium of transfer is not implied in the doctrine of transferrence. But, while conceding this, I wish to point out that this error no more affects my conclusions than a typographical error would. The point to which my whole discourse was actually directed was that of thought-transferrence through any hitherto unrecognized channel, whether material or not. In other words, I inquired whether the observed phenomena required the admission of any new law of nature in order to explain them.

Your other criticism is in these words: "He places much emphasis, for instance, on the extreme rarity of thought-transferrence in the ordinary course of life, and implies, somewhat sarcastically, that it ought to be much more frequent."

I can find in my written paper no justification for any such remark, and cannot even guess what passage it refers to. I did, indeed, point out the well-known and obvious fact that very rare phenomena become frequent when we learn how they are produced, or how they may be observed, and remarked, that, were thought-transferrence real, we should expect to learn how to produce it at pleasure as its conditions became better known. The great fact which I pointed out is this: after three years of painstaking labor by the English society, and one year of our own, no one shows us how to produce or observe thought-transferrence, nor indeed tells us any thing about it that we did not know before.

Professor James's remarks in *Science* of Feb. 5, are directed mainly to certain reflections upon the English society, for which I am not responsible to any further extent than as having made the remark which led to them. At the same time the question seems to me not devoid of interest. The ground which I take is, that the parts of the reproduced figures made by blindfolded percipients fit together in a way which could scarcely have been possible unless the percipient either saw the drawing he was making or had a knowledge of his work by some agency unknown to science. Professor James is not ready to concede this, but apparently claims that the muscular sense would have proved a sufficient guide, and suggests that I try the experiment myself. I beg leave to assure him that I did not venture on my conclusions until I had tried it. I cannot make any such drawings as those given on pp. 89 and 95 of the Proceedings of the English society by the muscular

sense. I should be interested to know from Professor James, whose superior knowledge of this subject I of course recognize, if others can do better, and if any blindfolded draughtsman at his command can make consecutively four such pictures as those on p. 95 with entire success, or can draw five lines out of six through the angles of an invisible hexagon as accurately as is done on p. 89. If so, my remark has no particular point. If not so, but if it be considered that the draughtsman must have seen the picture as he was drawing it, then the fact will be more valuable for what it suggests than for what it proves. It will suggest the question why the committee who conducted the experiments laid such stress on the percipient being blindfolded when he could in fact see.

S. NEWCOMB.

Sea-level and ocean-currents.

One has so little practice in differing from Professor Ferrel that it is difficult to know how to begin; but there are some points in his recent letter on 'Sea-level and ocean-currents' (*Science*, Jan. 22) that do not carry conviction. The first is, that the small head of water resulting from the superficial difference in temperature of the ocean in high and low latitudes should be as effective as he claims it to be in producing ocean-currents, and especially in producing the existing surface currents whose circuits seem to be so nearly completed without descending to great depths; for the supposition that there is a gradual rising-up of deep water at the equator in any thing like sufficient volume to feed the currents that flow thence towards the poles is not warranted by the known distribution of surface or deep-water temperatures. Professor Ferrel ascribes the origin of the southward return current from France past the African islands to an elevation of the sea-level on the western coast of Europe, where it is heaped up by the eastward pressure of the North Atlantic drift; but the homologue of this current in the South Atlantic is a well-marked stream that turns towards the equator, although it finds no land-barrier to its eastward passage beyond the Cape of Good Hope. According to the convectional theory, it is not needed at the equator, for the water that it supplies to the Gulf of Guinea ought to rise there from the abysses: it seems preferable to refer it to the winds, with which it accords very well, provided there is reason for thinking that the winds could carry it.

The effect of the winds seems to be visible in changing the direction of the currents in the Indian Ocean with the changes of the monsoons, and in altering the area of the counter-current of the equatorial Atlantic as the position of the trade-winds shifts with the seasons. A brief examination of Strachan's charts of the 'Currents and surface temperature of the North Atlantic Ocean,' published by the British meteorological committee, 1872, shows the mean velocity of the return current between Portugal and the Azores (latitude 37°.5 to 40°) to be seventeen miles a day in the four cold months, and only nine miles for the hot months. The winter average is based on forty-one determinations; the summer average, on ninety-eight.

The sufficiency of prevailing winds to establish deep currents has been discussed by Zöppritz, with results that are approved so far as I have read. His paper on 'Hydrodynamic problems in reference to ocean-currents' (Wiedemann's *Annalen*, iii., 1878, 582) furnishes a basis for the following statements. If an ocean of great depth acquire a certain velocity of motion at the surface, it will take 239 years to gain half this velocity at a depth of 100 metres; at the same depth, even a tenth of the surface velocity will not be reached for 41 years; at a depth of ten metres the times will be 2.39 and 0.41 years. But, given sufficient time, the effect of a continuous horizontal surface motion will be felt to the bottom, the velocity finally attainable decreasing with the increase of depth. From this it appears that the effect of any variations from the prevailing forces (winds) applied at the surface will be propagated downwards very slowly, and that below a very moderate depth the motion of the greater mass of the current will depend on the mean direction and velocity of motion in the surface water. To establish the currents as they now exist would require something like 100,000 years (pp. 598, 601, 607). According to Zöppritz, therefore, we should not expect to find significant changes of level in Lake Ontario as a result of our frequently shifting easterly and westerly winds; nor in the Atlantic, on account of the difference in the velocity of the wind, winter and summer. The attitude of the greater mass of water must be in both cases adjusted to the action of the mean annual winds. In view of these and other reasons, it does not seem probable that the 'strongest winds have no sensible effect' on the ocean-level and the ocean-currents. Doubtless both gravitative convection and wind friction have a share in causing the surface currents, but the latter has the larger.

W. M. DAVIS.

Cambridge, Jan. 31.

Association of sound and color.

A friend who is peculiarly sensitive to music tells me that in listening to an orchestra he invariably sees a brilliant yellow star when the triangle is struck, and a bluish green circle (hollow) at the clash of the cymbals. As I understand him, these appear instantaneously, and then fade out little by little. I should be glad to know whether any of the readers of *Science* have similar experiences.

BRADFORD TORREY.

Boston, Feb. 9.

Tadpoles in winter.

In response to the inquiry of H. M. Hill in *Science*, vii. No. 157, I would say that for the last ten years we have been able to get tadpoles in the small streams on the Ithaca flats just before they were covered with ice in the autumn, and as soon as the ice had disappeared in the spring. There has been no trouble in keeping them alive in an aquarium in the laboratory through the winter. Those so kept have transformed, and have proved to be tadpoles of Rana catesbiana, the common bullfrog.

S. H. GAGE.

Anat. lab. Cornell university, Feb. 8.

In the frozen marshes surrounding Fresh Pond, Cambridge, I saw a large number of tadpoles under the ice, and in the clear water around the edges, about the last of January. The weather for a few days previous had been very warm for winter, but this had been preceded by very cold weather. I had always supposed, as your correspondent, Mr. Hill, does, that they were only found in warm weather, and I was considerably puzzled.

WM. A. FORD.

Boston, Feb. 9.